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tinct from the usual geographical divisions of the globe.

All these facts are then shown by Mr. Wallace to be a necessary result of the "law of evolution." The nature and amount of "variation" are exhibited by a number of curious examples; the origin, growth and decay of species and genera are traced, and all the interesting phenomena of isolated groups and discontinuous generic and specific areas are shown to follow as logical consequences.

The remaining subjects discussed by Mr. Wallace carry him into the realm of fierce controversies, and relate to theories involving problems awaiting further investigations for their solution. One of these subjects—The Position of the Great Oceans and Chief Land Areas—is dealt decisively by Mr. Wallace, who claims that "on the whole they have remained unchanged throughout geological time." This declaration of the author has been already challenged, and we shall watch with interest if Mr. Wallace is capable of maintaining his position on this subject.

Perhaps the most valuable part of this work is the discussion of the question of geological time as bearing on the development of the organic world, leading to an investigation as to the exact nature of past changes of climate.

In answer to those who may consider the subject last spoken of as unsuited to such a work as the present, the author claims that, although many of the causes introduced are far too complex in their combined action to enable us to follow them out in the case of any one species, yet their broad results are clearly recognizable, and we are thus enabled to study more completely every detail and every anomaly in the distribution of living things, in the firm conviction that by doing so we shall obtain a fuller and clearer insight into the causes of nature, and with increased confidence that the "mighty maze" of Being we see everywhere around us is "not without a plan."

No person should offer an opinion on the "theory of evolution" who has not studied this work of Mr. Wallace, for it forms an essential part of the literature of the subject.

NOTE IN REGARD TO "PRIMITIVE DESIRES."

In a communication published in an earlier number of "SCIENCE," (No. 29, Jan. 15, 1881) Dr. Clevenger, of Chicago, discusses the relation existing between the desire for food, and the desires connected with the multiplication of the species. He appears to draw the conclusion that hunger is the primitive desire.

There are some observations made by alienists, which strongly tend to confirm Dr. Clevenger's theory.

It is well known that under pathological circumstances, relations obliterated in higher development and absent in health, return and simulate conditions found in lower and even in primitive forms.

An instance of this is the *pica* or morbid appetite of pregnant women, and hysterical girls for chalk, slate pencil and other articles of an earthy nature. To some extent, this has been claimed to constitute a sort of reversion to the oviparous ancestry, which like the birds of our day sought the calcareous material required for the shell structure in their food (?)

There are forms of mental perversion, properly classed under the head of the degenerative mental states, with which a close relation between the hunger appetite and sexual appetite becomes manifest.

Under the heading "Wollust,"—Mordlust-Anthropophagie" Krafft. Ebing describes a form of sexual perversion, where the sufferer fails to find gratification unless he or she can bite, eat, murder or mutilate the mate. He refers to the old Hindoo myth of *Çiva* and *Dûrgâ* as showing that such observations in the sexual sphere were not unknown to the ancient races.

He gives an instance, where after the act, the ravisher butchered his victim, and would have eaten a piece of the viscera, another where the criminal drank the blood and ate the heart, still another where certain parts of the body were cooked and eaten.*

In reference to this question, Dr. Clevenger some time ago sent me the following interesting letter, which, anticipating much that I should otherwise say, may find a place here.

CHICAGO, February 17, 1881.

Dear Doctor:

The suggestions that you made, in a recent note to me, on the extension of the Hunger Theory to Man, are of too much value not to be published. Professor E. D. Cope kindly sent me the reprint of an article of his entitled "The Origin of the Will" which appeared in the *Penn Monthly*, for June, 1877, wherein the Professor takes the ground that Hunger is the primitive desire. "The movement of the Amœba in engulfing a Diatom in its jelly is as much designed, as the diplomacy of the statesman or the investigations of the student, and the motive may be the same in all three cases; viz.: hunger" (p. 438). "In the lowest animal the first movement was doubtless a mere discharge of force; but the first designed action, the appropriation of food, was due to a sense of want or hunger, which is a form of pain. This was followed by gratification, a pleasure, the memory of which constituted a motive for a more evidently designed act, viz.: pursuit" (p. 446). I am rather inclined to reverse the conception of the unconscious being derived from the conscious act and conclude that the pain of hunger is akin to the desire barium may have for sulphuric acid or any molecule may have for another.

Yours truly,

S. V. CLEVINGER.

I cannot see the necessity of considering "the movement of the Amœba, as designed as the diplomacy of the statesman etc." It is either a truism according to one reading, or utterly erroneous—according to another. If "as designed" in the above means—based on the same broad summation of registered impressions potent in intellectual activity, I must say that due regards have not been paid to very fundamental facts in framing the clause criticized.

E. C. SPITZKA.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of "SCIENCE:—"

In reply to the remarks made by Mr. Morris about my communication to you (No. 43), I would like to say a few words.

In the first place I beg to enter a protest against the gentleman's suggestion with which he prefaces his reply, to wit:

"The main difficulty seems to be that I have gone counter to certain authors whom they are disposed to consider as authorities," meaning Prof. Dolbear and the writer. As to this objection, so often raised at the present moment, it seems to me that it is only applicable in case the authority is adduced in place of an argument, or in order to fortify it. As a rule, men of an independent turn of mind do not believe or accept theories because this or that authority has advanced them, but because they are plausible to them—perhaps only as long as they do not hear of any other in regard to the subject. But, if they should adopt another theory in place of one formerly held, it is certainly not on account of the fact that it has emanated from a certain authority, but because their mode of thinking and working out problems agrees with that which originated the theory, *i. e.* the authority's.

Since I have nowhere in my letter quoted any authority specifically, gathering my arguments from the works of those men whose writings are most congenial to my frame of mind, and from them weaving the net of my intellectual product with an occasional glimpse from my

* Ueber gewisse Anomalien des Geschlechts-tribes. Von Kraft-Ebing, Arch f. Psychiatrie VII.

own brains, I would seem to be justified in resenting this peculiar argumentation.

I might, in view of this unjust criticism, retort that perhaps it is altogether a better way to rely on an occasional authority, a good number of whom are towering up high above the sea of opinions as trustworthy beacons of light, than to steer along without looking up to them as guides now and then, and perhaps be wrecked on some unknown shore or unsuspected reef. The tendency to scoff at authorities because they are authorities, is just as pernicious as that to put faith in them for this same reason only.

As to my somewhat confused idea of heat, of which Mr. Morris takes the liberty to speak, I confess that I have supposed he understood the difference between radiant and conducted heat,* and he also was aware what was understood by universal consent with the expression "work." I should not have undertaken this discussion on physical subjects had I not been convinced that the terms to be used were agreed upon. However, Mr. Morris seems to be in a fair way to come down to the very last questions about the nature of motion and matter.

As to "*latent heat*," if Mr. Morris, Sir Wm. Thompson,* and many others persist in calling heat that which is not heat, they are at liberty to do so; yet they are wrong.

This I have conclusively shown, and Mr. Morris has not even tried to argue on it. Nor has he thought necessary to argue in regard to my remarks on his erroneous conception of the action of gravity. He only reiterates his assertion that the energy with which a body weighing a million pounds would fall on a body weighing one pound is the same. In order to prove this he says "we must add," and add and add, and then *one* will develop just as much energy as a *million*!

It seems futile to argue longer on a proposition that is in direct conflict with Newton's first law. If Mr. Morris has no room for the latter in his Universe, I must respectfully decline to enter it, preferring to stay outside of it in company with Sir Isaac and various others equally sound and reliable.

If Mr. Morris says motion is motion and cannot possibly become anything else, he is certainly right; but he forgets that there are certain *forces* for which we have as yet not been able to *prove* conclusively that they are motions. Of course, Mr. Morris has told us how he conceives of this relation between gravity and molecular motion, so called. (And there is cohesion and magnetism yet to account for.) But his explanations are wide away from the mark, which lies in an entirely different direction.

The combined action of all the radiant energy emanating from an infinite number of celestial bodies is transmitted in every direction through the Universe, and by oscillations, vibrations, and undulations of the attenuated matter (*not ether*—there is no ether!) which fills the interstellar spaces. In striking the surface of the various orbs, great and small, it exerts a uniform pressure, gravity.

Respectfully,
GEO. W. RACHEL, M. D.

NEW YORK, May 30, 1881.

THE "*Astronomische Nachrichten*."—It is announced that after the termination of the current volume, by authority of the Prussian Government, a new arrangement for the management of this journal will take effect. It will be edited by Prof. A. Krueger, the director of the Observatory at Kiel, in co-operation with the president of the "*Astronomische Gesellschaft*," of which association it will become a recognized organ.

* SCIENCE, Vol. I. p. 245. L. 24 fr. below.

** Admits that *t* is not heat, but favors the expression for convenience.

To the Editor of SCIENCE:

I can scarcely permit such curious statements as made by Prof. A. E. Dolbear, to pass unnoticed. In "SCIENCE" No. 43, he says:—"The decaying stump that shines by night, has a temperature not appreciably higher than surrounding objects." Can it be possible that he compares the state of matter in ancient wood, with the inconceivably rare gas whence Neptune was formed? Several cubic miles of it only weighed a grain, as has been proven by Helmholtz. It was in dissociation, no two atoms touched, therefore we assert with reason that it was absolutely cold and dark. The atoms in the stump had been in intimate association; indeed their organization was once so complex as to have been endowed with that most mysterious of all entities—LIFE!

When decaying, it was surrendering the force whose work organized it, and its faint luminosity was a portion thereof. The light was a result of preceding work, but in interstellar space, where atoms were yards apart, no previous work had been performed, and no force evolved whether heat, light, or any other save gravity and the slowest radial motion possible.

EDGAR L. LARKIN.
NEW WINDSOR OBS., Ill., June 13, 1881.

REPLY TO DR. J. J. MASON'S LETTER.

The writer of the review referred to, states that notwithstanding the construction which Dr. J. J. Mason now desires to see placed upon his words, the most careful reader would fail to draw any other conclusion from Dr. Mason's article, than that it was written in support of the theory that large cells are motor, and that sensory cells are small. It is true as Dr. Mason states that the sentence just preceding the one quoted in his letter refers specifically to the spinal cord of the turtle. But it is none the less true that the whole paragraph polemizes against a statement of Stieda's that the observations "have great weight against the conclusion that only the large nerve cells are connected with motor fibres," as not representing the ordinary view. In the earlier part of his article, Dr. Mason indeed goes so far as to question the statements of our best cerebral anatomists that certain very large cells are connected with the auditory, *i. e.* a sensory nerve, and this in obedience to the same theoretical bias which is manifested a few lines further on in this wise. "I would suggest, however, to those who may feel disposed to regard these cells (large cells of auditory nucleus and oblongata) as connected with the sense of hearing, that such a view involves giving to this apparatus in its central portion, a structure almost identical with one universally admitted to be motor, like, for example, that concerned in raising the lower jaw; whereas in the central structures for vision and olfaction *the cells are all very small.*" (Italics are own.) What other than the size of the cells and their nuclei does Dr. Mason refer to when he speaks of a "structure universally admitted to be motor?" Especially when it is borne in mind that immediately after he claim that all sensory cells are very small. In view of all this Dr. Mason's statement that no such claims as the one imputed to him by the reviewer had ever been made by him "in any form by hint, inference or otherwise," must have been penned in strange forgetfulness of what he has laid down in his published article. The reviewer can only interpret the remonstrance as an abandonment by Dr. Mason of his previous position. Every statement in the quoted paragraphs is simply erroneous, and to bring Dr. Mason face to face with facts that he has questioned, the reviewer refers to Dr. Mason's statement that the cells connected with vision "*are very small,*" and the reliable findings of Professor Packard, who happened to state that in the locust these cells are very large in relation to the other cells of the nervous system.

R. C. S